XXVIII. *Experiments and Obſervations on various Phænomena attending the Solution of Salts: By* R. Watſon, *A. M. F. R. S. Fellow of* Trinity College, *and Profeſſor of Chemiſtry, in the Univerſity of* Cambridge.

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HAVING lately had occaſion, in ſome chemical enquiries, to make various ſolutions of ſalts, I met with ſome phænomena, which did not appear to me either to have been ſufficiently attended to, or conſiſtently explained by writers upon that ſubject. The ſuſpenſion of ſalts in water, of metals in acids, of ſulphur in oils, and of other bodies in menſtruums ſpecifically lighter than the bodies themſelves, hath ever been conſidered in chemiſtry, as a problem of difficult ſolution. Thoſe philoſophers who acquieſce, upon the whole, in the cauſe which hath been aſſigned for this phænomenon by Sir Iſaac Newton, in his optical Queſtions, have taken great pains to illuſtrate the manner how it is effected, by ſuppoſing that the bodies are received into the pores of their reſpective menſtruums, and there kept ſuſpended by the attraction or, as Bernouilli and Freind would have it, by the reſiſtance ariſing from the tenacity of the fluid. Hence it happens, ſay theſe philoſophers, that after water is ſaturated with one ſalt, it is ſtill capable of diſſolving ſomewhat of a ſecond kind, and being ſaturated with that, of a third, and ſo on; juſt as a veſſel filled as full as poſſible with ſpheres or cylinders of one magnitude hath a capability of receiving ſimilar bodies of an inferior ſize, or bodies of a different figure. The opinion of Gaſſendus ſeems to have been generally adopted; he endeavours to prove, from the experiment which hath been mentioned, not only the poroſity of water, but a diverſity in the figures of the pores: *Affero & aliud experimentum ſingulare, quo viſus ſum mihi deprehendere interſperſa hujuſmodi ſpatiola inania intra aquam dari.—Aiebam, cum ſint ſalis corpuſcula cubica, poterunt ea quidem replere ſpatiola, quæ & ipſa cubica fuerint; at cum non modo commune ſal, ſed alumen etiam, quod eſt octahedricum, halinitrum item, & ſal ammoniacum ſaccharumque & alia quæ aliarum ſunt figurarum eâdem aquâ exſolvi poſſunt; erunt ergo etiam in aqua ſpatiola octahedrica atque id genus alia; adeo ut aqua, tametſi ſale ſaturata fuerit, nihilominus & alumen et cetera omnia exſolvere poſſit ac in ſeſe transfundere.* Gaſ. Phyſ. l. i. ſect. 1. cap. iii. The reaſon why warm water diſſolves in general more ſalt than cold water, ſeems as if it might be derived from the ſame principle, was it true; the interſtices between the elementary particles of water are enlarged by the expanſion of the fluid, and might therefore be ſuppoſed capable of admitting into them a larger quantity of ſalt. This doctrine hath been embraced by moſt philoſophers, eſpecially by the late Abbé Nollet, in the 4th volume of his Leçons de Phyſique; and I do not know that it hath been oppoſed by any body. The late Mr. Eller, of Berlin, hath carried this ſpeculation ſo far, as to publiſh a Table in the Berlin Memoirs for 1750, exhibiting the ſeveral quantities of above twenty different kinds of ſalt, which a given quantity of water will abſorb into its pores, without being in the leaſt augmented in bulk. It is not therefore without ſome uneaſineſs that I find myſelf conſtrained to diſſent from the general opinion, and particularly to differ from Mr. Eller, who hath treated this ſubject *ex profeſſo*; who made his experiments, as he himſelf aſſures us, with the greateſt exactneſs; and who was led by them to the diſcovery of what he is pleaſed to call, *une verité inconteſtable, ſavoir, que les plus petites parties conſtituantes de l'eau ſont doüees de pores ou d'interſtices dans leſquels les atomes de ſel peuvent nicher, ſans augmenter leur volume.* I do not at preſent ſee any very probable method of reconciling the different reſults of our enquiries; I will therefore content myſelf with giving a plain relation of the experiments which I have made upon this ſubject.

EXPERIMENT I.

I took a large mattraſs, containing, when filled to the middle of its neck, 132 ounces of water, Troy weight; the diameter of the cavity of the neck was ſix lines: having with a diamond marked the place where the water ſtood in the neck of the mattraſs, I dropped into it a ſingle piece of purified nitre, the weight of which was a 2600th part of the weight of the water, and immediately obſerved that the water was conſiderably elevated in the tube: during the ſolution of the ſalt, the water ſunk near one third of its whole elevation; but when the ſolution was entirely finiſhed, it remained very ſenſibly raiſed above the mark: ſo that, even from the experiment with this inſtrument, we may be aſſured that water cannot abſorb $\frac{1}{2000}$th part of its weight of nitre, without being augmented in bulk. Mr. Eller, from his experiments, concludes, that eight ounces of water will abſorb one drachm and a half, or above a 42d part of its weight of nitre; and hence I ſuppoſed the quantity of water which I uſed would have abſorbed above ſixteen times as much, or above 3 ounces; whereas the event ſhewed that it could not abſorb $\frac{1}{20}$ of an ounce. From the ſinking of the water during the ſolution, I was at firſt inclined to believe that ſome part at leaſt of the nitre was taken into the pores of the water: in order to ſee whether this conjecture could be verified by fact, I made the following experiment.

EXPERIMENT II.

I choſe two mattraſſes of unequal ſizes, containing quantities of water in the proportion of 12 to 1, the diameters of the necks being equal: into the largeſt I put $\frac{1}{650}$th part of the water's weight of nitre, and an equal quantity into the ſmaller; and I obſerved that the water, as well before as after the ſolution, was equally elevated in them both: this experiment was repeated. Now, if a given quantity of water can abſorb into its pores, without being increaſed in magnitude, any quantity of ſalt however ſmall, it ſeems reaſonable to ſuppoſe that a quantity containing twelve times as many pores ſhould abſorb twelve times as much, (ſince it is an allowed fact that the minuteſt portion of a ſalt is uniformly diffuſed through the largeſt quantity of water) and it might conſequently be expected, that the water ſhould riſe higher in the neck of the ſmaller matraſs than in that of the larger, which is contrary to the experiment.

EXPERIMENT III.

Apprehending that common pump water, with which I had made the preceding experiments, might have its interſtices preoccupied by ſelenites and other heterogeneous matters, and be thereby rendered incapable of admitting into them any additional ſubſtance; and obſerving that Mr. Eller had uſed in all his experiments 8 ounces of diſtilled water, I had hopes to have reconciled my experiments to his by that means: but upon trial, with diſtilled water, I found the elevation preciſely the ſame as before. Nor do the concluſions depend upon the kind of ſalt; they hold true *mutatis mutandis* of any other ſalt as well as nitre. During the ſolution the water is refrigerated and thereby contracted in magnitude, and the ſmaller the quantity the greater will be the cold and conſequent contraction produced by the addition of ſmall portions of ſalt; but I cannot ſuppoſe that this circumſtance could be overlooked by Mr. Eller, though it induced me to uſe a much larger quantity, or that he attributed the ſinking of the water during the ſolution, to an imbibition of the particles of the ſeveral ſalts into the pores of the water, and thence by calculation conſtructed his table.

EXPERIMENT IV.

Having always remarked that the water in the neck of the matraſs was elevated higher upon the firſt immerſion of the ſalt, than after it was wholly diſſolved, I endeavoured to aſcertain the difference in ſeveral kinds of ſalt. To do this with the greater exactneſs, I pitched upon a matraſs which had a neck as far as I wanted it accurately cylindrical, as I found by obſerving the elevations occaſioned by the additions of equal portions of water; the matraſs held about 67 ounces of water. The ſalts I uſed were all dry, and in as large pieces as the neck of the matraſs would admit; the water was heated to the forty ſecond degree of Fahrenheit's thermometer, and kept as nearly as could be in that temperature. I changed the water for each experiment, and uſed in each 24 penny weights of ſalt; the heights to which the water roſe, as meaſured from a mark in the middle of the tube, before and after the ſolution of each ſalt are expreſſed in the following table: the firſt column denotes the height to which the water was elevated by 24 penny weights of ſalt before its ſolution, the ſecond after its ſolution, the third the difference in fractional parts of the elevation before ſolution

Elevation by 24 penny weights of ſimple water	0	58	
24 penny weights of genuine Glaubers ſalt	42	36	$\frac{1}{7}$

Vol. ſalt of ſal. ammon.	40	33	$\frac{7}{40}$
Sal ammon.	40	39	$\frac{1}{40}$
Refined white ſugar	39	36	$\frac{1}{13}$
Coarſe brown ſugar	39	36	$\frac{1}{13}$
White ſugar candy	37	36	$\frac{1}{37}$
Glaubers ſalt from Lymington	35	29	$\frac{6}{35}$
Terra foliata tar.	37	30	$\frac{7}{37}$
Rochelle ſalt	33	28	$\frac{5}{33}$
Alum not quite diſſolved	33	28	$\frac{5}{33}$
Borax not half diſſolved in 2 days	33	31	$\frac{2}{33}$
Green vitriol	32	26	$\frac{3}{16}$
White vitriol	30	24	$\frac{1}{5}$
Nitre	30	21	$\frac{3}{10}$
Sal gem. from Northwich	27	17	$\frac{10}{27}$
Blue vitriol	26	20	$\frac{3}{13}$
Pearl aſh	25	10	$\frac{3}{5}$
Vitriolated tartar	22	11	$\frac{1}{2}$
Green vitriol calcined to whiteneſs	22	11	$\frac{1}{2}$
Dry ſalt of tartar	21	13	$\frac{8}{21}$
Baſket ſea ſalt	19	15	$\frac{4}{19}$
Corroſive ſublimate	14	10	$\frac{2}{7}$
Turbith mineral	9	0	

Had I not been in ſome meaſure perſuaded, from the reſult of the preceding experiments, that no portion of any ſalt could be abſorbed into the pores of water, I ſhould have readily concluded that the third column of this table denoted ſuch parts of 24 penny weights of the ſeveral ſalts as might be lodged in the interſtices of 67 ounces of water without increaſing its magnitude: the quantities indeed which might have been thus aſcertained would have but ill agreed with thoſe which are determined by Mr. Eller; and that diverſity of quantity may ſuggeſt a doubt concerning the validity of his principle. The ſinking of the water in the neck of the matraſs ſeems to be a general phænomenon attending the ſolution of all ſalts; the quantity of the deſcent is various from $\frac{1}{40}$ to $\frac{1}{2}$ of the whole elevation in thoſe ſalts which I have tried. In forming the table, I repeated many of the experiments, but found no variation which could affect the general concluſion; with particular attention I repeated the ſolution of vitriolated tartar, for I thought it a very remarkable citcumſtance that one of the hardeſt ſalts ſhould be more diminiſhed in proportion to its whole bulk than any other, but the numbers in the table 22 and 11 accurately expreſſed the height before and after ſolution upon the repetition of the experiment, ſo that it may be relied upon as a certain fact that a cubic inch of vitriolated tartar is by ſolution in water reduced to half a cubic inch, though the water cannot, as appeared from an experiment I made, abſorb $\frac{1}{1000}$th part, nor, as I believe, any part, of that ſalt without being augmented in magnitude. It is evident from the table that ſal gemmæ, blue vitriol, corroſive ſublimate, calcined vitriol, and in general thoſe ſalts which retain the leaſt water in their compoſition and conſtitute the hardeſt maſſes, ſink more in proportion to their reſpective bulks than any other. I own myſelf at a loſs for a general principle to explain this general phænomenon, unleſs the air contained in the ſeveral ſalts may be eſteemed ſufficient for the purpoſe; a very copious ſeparation of air from the ſalts during the whole time of their ſolution may be readily obſerved in all of them, and a ſmall portion of it, combined with the particles of a ſalt, may augment its bulk, without ſenſibly increaſing its weight. Yet the two following experiments rather tend to diminiſh the probability of this opinion.

EXPERIMENT V.

I took water which had been well purged from its air by long boiling, and which had been corked up whilſt it was warm; when it had acquired a proper temperature, I filled a matraſs with it, as before, and putting into it ſal gemmæ, &c. I obſerved that the elevation before ſolution was the ſame as when common water was uſed, and that it ſunk equally in the neck during the ſolution; but then the ſeparation of air ſeemed greatly leſs in all the trials I made. This phænomenon is eaſily explained: common water is always ſaturated with air; upon the addition of any ſalt, the particles of water begin to attract and diſſolve the ſalt, and let go the air with which they are united; this air, added to the air contained in the ſalt, renders the whole much more viſible in common than in boiled water. Muſſchenbrook and others are of opinion, that air only fills the interſtices of water, without augmenting its bulk; they ground their opinion upon obſerving that the ſpecific gravities of common water and of water purged from its air are equal; the fact, taking it for granted, will ſcarcely authorize the concluſion: for, ſuppoſing that a cubic inch of common water contains even a cubic inch of air, the difference of the weight of the water when ſaturated with air, and when freed as much as poſſible from it (though probably it can never be wholly freed from it), will not equal $\frac{1}{4}$ of a grain: how imperceptible then muſt the difference be, if water, inſtead of an equal bulk, doth not contain $\frac{1}{100}$th part of its bulk of air, which is a ſuppoſition much nearer to the truth: the air is ſeparated from the water during the ſolution of the ſalt, and the particles of the ſalt probably occupy its place as happens in other chemical precipitations; but we cannot thence infer that they are received into the interſtices of the water, unleſs we had more concluſive arguments, to prove that the air itſelf was lodged in them. I varied the preceding experiment by putting two equal and tranſparent pieces of ſal gemmæ into two tall drinking glaſſes, filled one with common, the other with boiled water; from the firſt there continually aſcended a very viſible ſtream of air, and the ſalt and the bottom of the glaſs were covered with bubbles, it ſeeming as if the water quitted its air to diſſolve the ſalt; in the other, though ſome air was ſeen breaking out from the ſalt whilſt it was diſſolving, there did not ſeem to be any precipitated, as it were, from the water. In moſt of the experiments which I made, the boiled water diſſolved a given quantity of ſalt ſooner than the common water, when they had the ſame degree of heat; but the difference in time might be owing to the different magnitude of the ſurfaces of the ſalt, though from the generality of the event, I ſhould rather attribute it to the different diſſolving powers of water, when replete with, and when deprived of air.

EXPERIMENT VI.

Thinking that the difference in the bulks of the water before and after ſolution might be owing to the ſeparation and eſcape of ſome volatile principle; I took care to balance as accurately as I could, water and ſal gemmæ, water and ſalt of tartar, water and vitriolated tartar, &c. and then putting the ſeveral ſalts into the water, I obſerved when the ſolution was accompliſhed, whether the equilibrium of the ſcales was affected, but I could not diſtinguiſh any change. Dr. Hales and others have ſpoken of the exiſtence of air in ſalts, and have in two or three inſtances inveſtigated the quantity, but after a very different manner from that I have uſed; nor can I think myſelf at liberty to eſteem this air which is ſeparated by ſolution, of the ſame nature with that which is called by him and others fixed air, inaſmuch as fixed air makes a conſiderable part of the weight of the bodies from which it is extracted, precipitates lime water, and is ſeldom diſcharged (or perhaps produced from ſome of the minute parts of the body being converted by the violence of the fire, &c. into an elaſtic fluid), except when the body is decompoſed; whereas this makes only a conſiderable part of the bulk of bodies, and thus diminiſhes their ſpecific gravity without ſenſibly increaſing their abſolute weight; does not, as I collected from ſome rough trials, render lime water turbid; and is ſet at liberty, though not by a mechanical diviſion, yet by an operation ſomewhat different from chemical decompoſition. It hath been remarked by ſome, that ſaline ſolutions will not cryſtallize without much difficulty in an exhauſted receiver; perhaps becauſe the particles of ſalt cannot attract that principle which ſhould cement them together, which at leaſt may be ſeen eſcaping from them when they begin to be ſeparated. Mr. Boyle obſerved, that aquafortis, poured upon a ſtrong vegetable alcali, did not cryſtallize till it had been long expoſed to the air (though I ſhould rather attribute this failure to the weakneſs of his aquafortis than to the want of air, ſince I have frequently, by uſing the fuming ſpirit of nitre, obtained cryſtals of an inch in length almoſt inſtantaneouſly); and ſeveral other phænomena might be adduced reſpecting the cryſtallization of ſalts, which ſeem to indicate the neceſſity of admitting air as a very efficacious inſtrument in producing that effect: but future experience may tend to elucidate this matter. Having uſed great attention in making the experiments from which the preceding table was compoſed; I thought I had a good opportunity of deriving from it the ſpecific gravities of the ſalts which are there mentioned. I accordingly calculated the following table; in the firſt column of which are expreſſed the ſpecific gravities as calculated from the increaſe of bulk before ſolution; in the ſecond, after the ſolution.

Genuine Glauber's ſalt	1,380	1,611
Cryſtals of kelp	1,414	1,467
Volat. ſalt of ſal ammoniac	1,450	1,787
Sal ammoniac	1,450	1,487
Sugar refined, brown, barley	1,487	1,611
White ſugar candy	1,567	1,611

Terra foliata tartari	1,567	1,933
Glauber's ſalt from Lymington	1,657	2,000
Rochelle ſalt	1,757	2,071
Alum	1,757	2,071
Borax	1,757	
Green vitriol	1,812	2,230
White vitriol	1,933	2,416
Nitre	1,933	2,766
Very tranſparent ſal gem, from Nortwich	2,143	3,411
Blue vitriol purified	2,230	2,900
Pearl aſh	2,320	5,800
Vitriolated tartar	2,636	5,272
Green vitriol calcined to whiteneſs	2,636	5,272
Dry ſalt of tartar	2,761	4,461
Baſket ſea ſalt	3,052	3,866
Corroſive ſublimate	4,142	5,800
Mercury diſtilled with acid of vitriol, and freed from its acid by a ſtrong fire	6,444	

The numbers in the firſt column correſpond very well, upon the whole, with the ſpecific gravities which have been determined by others hydroſtatically; thus the ſpecific gravities of nitre, alum, white and green vitriol, ſal ammoniac, ſal gemmæ, &c. are greater than what are aſſigned to theſe bodies by ſome authors, and leſs than what have been determined by others; it ſeems as if the ſpecific gravities of ſaline bodies might, in a proper veſſel, be more accurately aſcertained from the obſerved increaſe of the water's bulk than any other way. Upon the ſuppoſition that the eſcape of the air is the reaſon of the water's ſinking during the ſolution, and that this air contributes little to the weight of the ſalts, though it may be abſolutely neceſſary to the exhibiting the ſaline moleculæ under a viſible cryſtalline appearance; the ſecond column will denote the real ſpecific gravities of the ſalts as freed from air. That this air is combined with the ſalts, and doth not ſimply adhere to their ſurfaces, may appear from hence, that the ſpecific gravities, as calculated from the increaſe of bulk obſerved in the water before ſolution, ſufficiently correſpond with thoſe which philoſophers have determined hydroſtatically: nor indeed, upon exhauſting the air from the ſalts, by an air pump, could I obſerve that it was ſeparated, in leſs quantity during ſolution.

EXPERIMENT VII.

Since equal quantities of ſalt muſt contain equal quantities of air, it might be expected *a priori*, if the eſcape of the air was the occaſion of the water's ſinking, that equal weights of ſalt would produce equal augmentations of bulk, and unequal weights augmentations proportionable to their weights; but, to be aſſured of this, I took a matraſs containing about 30 ounces of water, the tube being cylindrical for about 7 inches in length. When the matraſs was filled to a proper mark, I put into it 7 pennyweights of powdered ſal gem.: the water after the ſolution had riſen through 17 tenths of an inch; by the addition of 14 pennyweights more, the water was raiſed through 51 diviſions from the firſt mark, or twice 17 from where it ſtood after the ſolution of 7

7 pennyweights. In the ſame matraſs I tried a ſimilar experiment with nitre; the water was raiſed through 10 diviſions, by 3 pennyweights of powdered nitre; and by 18 more, it ſtood after the ſolution at the 70th diviſion from the firſt mark, and conſequently roſe through ſix times the ſpace, through which it had been raiſed by 3 pennyweights. From theſe, and other experiments of the ſame kind, I am diſpoſed to believe that equal portions of ſalt produce equal augmentations in the bulk of the water wherein they are diſſolved; at leaſt, this holds true when the ſalt diſſolved bears but a ſmall proportion to what would be requiſite to ſaturate the water. But, in making this experiment, great care muſt be taken to keep the ſalts of the ſame dryneſs; I had once tried it with three equal quantities of ſea ſalt, and arrived at a quite different concluſion; the increaſes of bulk occaſioned by the ſolution of the ſeveral ſalts being ſeparately taken, as 15, 16, 17, but the ſalt being much drier than the air in the laboratory, had undoubtedly attracted the humidity, and that portion had attracted the moſt which had been the longeſt in it, and which was laſt diſſolved. Nor ſhould the temperature of the water be neglected; a ſenſible error may proceed from a minute change in that. This experiment confirms the firſt, for, was any part of ſalt abſorbed into the pores of the water, it certainly ought to be expected that the elevation occaſioned by the ſolution of 3 pennyweights of nitre ſhould be leſs than $\frac{1}{6}$th of that occaſioned by 18 pennyweights, and yet I found it to be accurately $\frac{1}{6}$th upon repeating the experiment with diſtilled water. It confirms it too in another view, 3 penny-

weights or $\frac{1}{200}$th part of the weight of the water, raiſed it through one inch; hence $\frac{1}{2000}$th part would have raiſed it through one tenth of an inch, which any eye may diſtinguiſh.

Dr. Lewis, for whoſe great abilities in chemiſtry I have a very high reſpect, in his little treatiſe upon American potaſhes, is of opinion, that the augmentation of the bulk of water doth not proceed uniformly, according to the quantity of ſalt added; and he forms his concluſion from obſerving, that the loſſes of weight ſuſtained by the ſame body in different ſolutions, were not uniform, but continually diminiſhed; the loſſes correſponding to ſeven ſucceſſive equal quantities being as 24½. 24. 23½. 22. 22. 21. 20. Upon conſidering this matter in a mathematical light, I an inclined to draw a quite different concluſion; but I will firſt mention ſome experiments which I had formerly made with a different view, and which agree very well with Dr. Lewis's.

EXPERIMENT VIII.

I had conceived that if, in a given quantity of water, ſeveral quantities of ſalt, increaſing in any arithmetical or geometrical progreſſion, were diſſolved; that the increments of ſpecific gravity would increaſe in the ſame progreſſion. In order to ſee whether this conjecture could be eſtabliſhed by experiment, I diſſolved in a given quantity of water, different portions of ſea ſalt, increaſing in the progreſſions expreſſed in the annexed tables, where the firſt column of each denotes the proportional quantities of ſalt in pennyweights, the ſecond, the loſs of weight of a given body in quarter grains; the third the exceſs of the ſpecific gravity of each ſolution, above the ſpecific gravity of water.

Tab. I.

	263	0
9	273	10
18	282	19
27	292	29
36	301	38
45	309	46

Tab. II.

	263	0
5	269	6
10	274	11
15	280	17
20	285	22
25	289	26
30	294	31
35	300	37
40	304	41
45	309	46
50	312	49
55	316	53

Tab. III.

	883	0
4	899	16
8	915	32
12	930	47
16	945	62
20	959	76
24	971	88
28	985	102
32	996	113
36	1009	126
40	1020	137

The difference of the numbers in the third column of each table from arithmetical progreſſions, is obvious at firſt view, the difference of the two laſt numbers of each being conſiderably leſs than the difference between the two firſt: and the numbers 6. 11. 22. 41. correſponding to the geometrical progreſſion 5. 10. 20. 40. in the ſecond table as well as the numbers 16. 32. 62. 113 correſponding to the geometrical progreſſion 4. 8. 16. 32, in the third, differ conſiderably from geometrical progreſſions, whoſe common ratio is $\frac{1}{2}$.

In making theſe experiments there are three obvious ſources of error: the heat may not remain conſtant; the additional weights of ſalt may not be accurately equal; and the weight of the given body may be more or leſs than what is expreſſed by any quantity leſs than ¼ of a grain; yet the differences of the preceding numbers, from arithmetical or geometrical progreſſions, are too great to be explained from any or all of theſe ſources taken together. We may obſerve that the loſſes of weight, correſponding to equal portions of ſalt, are, upon the whole, diminiſhed; but it will not follow from thence that the bulks are not equally augmented. For, ſince the ſpecific gravity of every body is properly denoted by a fraction, whoſe numerator expreſſes the abſolute weight, and denominator the magnitude of the body; let $\frac{w}{m}, \frac{w+x}{m+y}, \frac{w+2x}{m+z}, \frac{w+3x}{m+s}$, &c. be a ſeries of fractions, whoſe ſeveral numerators expreſs the weights of a given quantity of water, as increaſed by the addition of equal portions of any ſalt denoted by x, and whoſe denominators expreſs the bulks of the water after the ſolution of each portion of ſalt, the increments of bulk being denoted by y, z, s; now let us ſuppoſe that the loſſes of weight ſuſtained by the ſame body, that is, the ſpecific gravities, increaſe uniformly, then will the above ſeries of fractions increaſe uniformly, let $\frac{w}{m}=a$; $\frac{w+x}{m+y}=a+b$; $\frac{w+2x}{m+z}=a+2b$; $\frac{w+3x}{m+s}=a+3b$, from theſe equations inveſtigating the proportion between y, z, s, which repreſent the augmentations of bulk, it will appear that $y:z::a+2b:2a+2b$, or in a greater ratio than that of 1 : 2 and that $z:s::2a+6b:3a+6b$ or in a greater ratio than that of 2 : 3, in which ratios they ought reſpectively to have been, had the denominators or the bulks of the fluid increaſed uniformly, when the ſpecific gravities or abſolute weights increaſed uniformly. We ſee from this, what concluſion ſhould have been formed, had the increments of ſpecific gravity from equal portions of ſalt been equal. Again, ſuppoſe that $\frac{w}{m}$, $\frac{w+p}{m+q}$, $\frac{w+2p}{m+2q}$, $\frac{w+3p}{m+3q}$, &c. denote a ſeries of fractions, whoſe numerators, expreſſing the weights of a given quantity of water as increaſed by the addition of ſalt, and whoſe denominators, expreſſing the bulks, both increaſe uniformly, then will the ſeveral differences between the 2d and 1ſt, between the 3d and 2d, and ſo on, be as $\frac{1}{m \times \overline{m+q}}$, $\frac{1}{\overline{m+q} \times \overline{m+2q}}$, $\frac{1}{\overline{m+2q} \times \overline{m+3q}}$, $\frac{1}{\overline{m+3q} \times \overline{m+4q}}$, &c. which fractions being inverſely as their denominators conſtitute a decreaſing ſeries; but the increments of ſpecific gravity from the addition of equal portions of ſalt, are proportionable to theſe fractions, and therefore ought perpetually to decreaſe, though we allowed the bulk of the compound to be preciſely equal to the bulk of the water and ſalt taken together, that is, though we allowed the bulk of the water to increaſe uniformly according to the quantity of ſalt added: now as it is evident from Dr. Lewis's experiments, and from each of the preceding tables, that the increments of ſpecific gravity do decreaſe upon the whole, when the abſolute weights increaſe uniformly, we may venture to conclude that the bulks increaſe uniformly alſo. I thought proper to explain the foregoing principle and to determine the ratio, becauſe the matter ſeems to have been miſtaken by many; however, it may be eaſily apprehended that the increments of ſpecific gravity, from the addition of equal quantities of ſalt to a given weight of water, ought perpetually to decreaſe; becauſe the difference between the ſpecific gravities of the water and of the ſalt perpetually decreaſes, as the water approaches to perfect ſaturation. In like manner, if to a given quantity of water we add any number of equal quantities of oil of vitriol, or any fluid miſcible with and heavier than water; the increments of ſpecific gravity will perpetually decreaſe, though they will never entirely vaniſh, becauſe there is a perpetual approximation to the ſpecific gravity of the acid, which yet the mixture can never acquire; and, *vice verſa*, if to water we add a lighter fluid, as ſpirits of wine by equal portions, the ſpecific gravity of the mixture will conſtantly decreaſe by unequal decrements; but the decrements will never vaniſh, becauſe the mixture muſt ever remain ſpecifically heavier than ſpirit of wine.

EXPERIMENT IX.

The quantities of various ſalts, which may be diſſolved in a given quantity of water, have been aſcertained by Boerhaave, Eller, Spielman, and others; their accounts differ ſomewhat from one another, as might be expected from the different temperatures of the air, the different ſtate of their ſalts; the different times (a circumſtance of no ſmall conſideration in this matter) which they allowed the water to act upon the ſalts before they concluded it to be fully ſaturated, and from ſome other circumſtances which might perhaps with advantage be taken into the account, and a more accurate table compoſed than hath hitherto been publiſhed; but as the differences would be ſmall, and might not tend to any new diſcoveries, I could not perſuade myſelf to be at the trouble of making the requiſite experiments. I thought it would be a more uſeful undertaking to determine the ſpecific gravities of ſaturated ſolutions of various ſalts. In compoſing the following table, I uſed every poſſible precaution; the ſolutions were fully ſaturated, by permitting the water to reſt upon the ſalts for ſome weeks, and frequently ſhaking the ſolutions during the interval: I had ſome reaſons for chuſing this method rather than the much ſhorter one of diſſolving the ſalts in hot water, and letting the ſolutions cool, though the event will be much the ſame in both ways; my balance was extremely ſenſible, though I did not uſe any weight leſs than a quarter of a grain; the water in which the ſalts were diſſolved was not ¼ of a grain in 890 heavier than diſtilled water; the ſolutions were all of the ſame temperature, Fahrenheit's thermometer ſtanding between 41 and 42° during the whole time of taking the ſpecific gravities.

A Table exhibiting the ſpecific gravities of water ſaturated with various ſalts. Thermometer 41—42°, barometer 30 inches.

Water in which the ſalts were diſſ.	1,000	Cryſtals of tar	1,001
		Arſenic	1,005
Saturated with quicklime	1,001	Borax	1,010
		Corroſ. ſublim.	1,037

Alum	1,033	Nitre purified	1,095
Genuine Glau. ſalt	1,052	Rochelle ſalt	1,114
Vitriolated tart.	1,054	Blue vitriol	1,150
Common ſalt	1,198	Green vitriol	1,157
Arſen. nitre	1,184	Sal gemmæ	1,170
Glau. ſalt Lyming.	1,232	Epſom ſalt Lym.	1,218
Sal ammon.	1,072	White vitriol	1,386
Vol. ſalt of ſal. am.	1,077	Pearl aſh	1,534
Cryſtals of kelp	1,087		

By making other tables ſimilar to the preceding, when the thermometer ſtands at 62°, 82°, 102°, &c. or when the heat increaſes or decreaſes in any known ratio; it is extremely probable that the law, according to which the diſſolving power of water varies with the variation of its heat, might be inveſtigated. I have ſome reaſons for thinking that though it increaſes with the increaſe of heat, yet it doth not increaſe in the direct ſimple ratio of the heat; but what the law is, or whether all ſalts follow the ſame law, I cannot, from any experiments I have already made, determine; and I have no leiſure at preſent to proſecute the enquiry. The concluſion will be unavoidably liable to a ſmall inaccuracy; for whether the ſpecific gravities be inveſtigated by weighing the ſeveral fluids in a given veſſel filled to a given mark, or by weighing a given ſolid in each of them, we ſhall not thence obtain the weights of equal bulks, ſince the containing veſſel or the ſolid, from the difference of the heats, have a different capacity or a different bulk. However, it is not apprehended that this circumſtance would ſenſibly affect the concluſion, eſpecially as it is ſubject to calculation and might be allowed for. It ought, at the ſame time, to be obſerved, that a given bulk of the water with which the ſpecific gravities are compoſed, will have different weights when the heats are different; and theſe differences ought firſt to be aſcertained.

EXPERIMENT X.

Having thus determined the ſpecific gravities of ſaturated ſolutions of ſeveral ſalts, in a given degree of heat; my next enquiry was to find the ſpecific gravities of water impregnated with a given quantity of the ſeveral ſalts: I accordingly diſſolved in 168 pennyweights of water, 14 pennyweights, or $\frac{1}{12}$ of the weight of the water of the eight following ſalts. The thermometer was at 40° and barometer at 29½.

A Table of the ſpecific gravities of water impregnated with $\frac{1}{12}$ of its weight of

Water	1,000
Sea ſalt	1,059
Blue vitriol	1,052
Nitre	1,050
White vitriol	1,045
Green vitriol	1,043
Glau. ſalts Lym.	1,039
Glau. ſalts Genu.	1,029
Sal ammon.	1,026

I could not have made this table much more extenſive, ſince in the 40th degree of the thermom.

water will not diſſolve $\frac{1}{12}$ of its weight of alum, borax, vitriolated tartar, corroſive ſublimate and a great many other ſalts; however, as ſuch a table cannot fail of being uſeful in chemical, and perhaps medical reſearches, it would be worth while to make it more general, either by diſſolving a leſs portion of ſalt, or making uſe of a greater degree of heat.

EXPERIMENT XI.

To theſe tables I have ſubjoined another of a different nature, wherein the ſpecific gravities of water impregnated with different quantities of the ſame ſalt from $\frac{1}{3}$ down to the 1024th part of the weight of the water, are determined. I cannot accuſe myſelf of careleſſneſs in making any of the experiments from which the table is formed; but part of it being made in a room where the heat was about 55°, and the other in my laboratory, when it did not exceed 46°, a certain inaccuracy, though it will be a very ſmall one and ſcarce ſenſible in the weight of the ſmall body which I uſed, will attend it upon that account. The ſalt was ſea ſalt of the fineſt kind, and extremely dry; many of the experiments were repeated.

A Table of the ſpecific gravity of water impregnated with different quantities of ſea ſalt. Thermometer between 46 and 55°.

Water	1,000
Salt $\frac{1}{3}$	1,206

$\frac{1}{4}$	1,160
$\frac{1}{5}$	1,121
$\frac{1}{6}$	1,107
$\frac{1}{7}$	1,096
$\frac{1}{8}$	1,087
$\frac{1}{9}$	1,074
$\frac{1}{12}$	1,059
$\frac{1}{14}$	1,050
$\frac{1}{15}$	1,048
$\frac{1}{16}$	1,045
$\frac{1}{18}$	1,040
$\frac{1}{21}$	1,032
$\frac{1}{24}$	1,029
$\frac{1}{27}$	1,027
$\frac{1}{28}$	1,025
$\frac{1}{30}$	1,024
$\frac{1}{32}$	1,023
$\frac{1}{36}$	1,020
$\frac{1}{39}$	1,019
$\frac{1}{42}$	1,015
$\frac{1}{48}$	1,014
$\frac{1}{54}$	,013
$\frac{1}{56}$	1,012
$\frac{1}{72}$	1,009
$\frac{1}{84}$	1,007
$\frac{1}{108}$	1,006
$\frac{1}{126}$	1,005
$\frac{1}{144}$	1,004
$\frac{1}{162}$	1,003
$\frac{1}{192}$	1,0029
$\frac{1}{256}$	1,0023
$\frac{1}{320}$	1,0018

$\frac{1}{448}$

$\frac{1}{448}$	1,0017
$\frac{1}{512}$	1,0014
$\frac{1}{848}$	1,0008
$\frac{1}{1024}$	1,0006

From this table it will be eaſy to determine how much the ſpecific gravity of water is increaſed by the ſolution of a given quantity of ſalt, and, *vice verſa*, if we know the ſpecific gravity of any ſolution of ſalt, we may form a good conjecture of the quantity of ſalt contained in it, which obſervation may be of ready uſe in eſtimating the ſtrength of brine ſprings, and of ſea water, taken up in different climates, or upon different coaſts in the ſame climate. Thus, if a ſalt ſpring, or ſea water, ſhould weigh $\frac{1}{50}$ more, bulk for bulk, than common water; we may conclude that it contains $\frac{1}{36}$ of its weight of ſalt; if $\frac{1}{40}$, it hath nearly $\frac{1}{28}$; if $\frac{1}{25}$, $\frac{1}{18}$; if $\frac{1}{20}$, $\frac{1}{14}$; and ſo on: we may always find limits near enough to form a concluſion from, though the exact number denoting the weight in any particular caſe ſhould not be met with in the table.

After I had drawn up the preceding account of the experiments which I had made, I received the Berlin Memoirs for 1762, publiſhed laſt year, in which there is a memoire entitled—*Experiences ſur le poids du ſel et la gravité ſpécifique des ſaumures faites et analyſées, par M. Lambert.* In this memoire, the very ingenious author hath made much uſe of the principle, which I have endeavoured to call in queſtion in the beginning of this paper; and hath calculated the different quantities of ſea ſalt, which are abſorbed into the pores of water, when a given quantity is diſſolved in different quantities of water. The admiſſion of this principle hath drawn him into ſome concluſions which ſeem not quite conſonant to true philoſophy; as when he aſſerts that the quantity which is abſorbed into the pores, is not proportional to the number of the pores or the quantity of water: for, if a given quantity of water, ſuppoſe *A*, will abſorb a given quantity of any ſalt, ſuppoſe *a*, I can ſee no poſſible reaſon why *mA* ſhould not abſorb *ma*: for imagining *m A* to be divided into portions reſpectively equal to *A*, and equal quantities of ſalt to be diſſolved in each of them; then, from the ſuppoſition, each of them will abſorb *a*; and when they are all mixed together, as no precipitation will enſue, the ſum, or *m A*, muſt have abſorbed *ma*. But I have no inclination to animadvert upon what ſeems to be a ſmall miſtake of an author, whoſe various writings do much honour to philoſophy in general, nor to involve myſelf in a diſpute with any one. The following experiment may perhaps be thought concluſive againſt the doctrine of ſalts being abſorbed into the pores of water: I took a large glaſs receiver, containing near ſix gallons; into its neck, by means of a hole bored through a cork, I cemented a ſmall glaſs tube; and having filled the whole up to the middle of the tube with water, I dropped in a piece of ſea ſalt, weighing leſs than one forty thouſandth part the weight of the water: the water inſtantly roſe in the tube, continued ſinking during the ſolution, but at laſt remained as much elevated as it would have been had there been no more water than what would have been ſufficient to diſſolve it. In making this experiment, the receiver ſhould not be touched by the hand, for its parts ſuddenly expanding themſelves occaſion an inſtantaneous ſinking of the water in the tube, as I have frequently experienced, and might thus induce a ſuſpicion of the water's not being elevated by the addition of ſalt. I would not be underſtood from theſe experiments to deny the poroſity of water, ſince philoſophers have thought that the paſſage of light through it, and other phænomena indicate the exiſtence of vacuities in it; but I cannot believe, however ſolution be carried on, that the ſmalleſt quantity of ſalt can be diſſolved in the largeſt quantity of water, without increaſing its magnitude. The cauſe of the water's ſinking during ſolution doth not appear to be ſo certain; the eſcape of air, to which all the appearances induced me to refer it, and to which it may perhaps ſtill be owing, ſeems to be liable to ſome objections, not only from the experiments I have before mentioned, but from the following.

EXPERIMENT XII.

I took two matraſſes of equal dimenſions, one filled with common water, the other with boiled water. I poured into them equal quantities of oil of vitriol; in the firſt there ſeemed to be an univerſal precipitation of air, as it were, from every particle of the fluid, which, by little and little, formed itſelf into larger bubbles, and aſcending through the neck, eſcaped; in the other, hardly any air could be obſerved, the water ſunk during the ſolution of the acid very apparently, yet $\frac{1}{1000}$th part of the water's weight of acid cauſed a ſenſible elevation: ſo that, whatever may be thought of the cauſe of the water's ſinking during the ſolution of a ſalt, the principle of its being to a certain degree imbibed into the pores of water ſeems in no caſe to be true, whether the ſalt be in a concrete or fluid form. This ſubject may receive ſome illuſtration from what is obſerved in the freezing of water; ice from common water is always ſpecifically lighter than water, from its retaining in its concrete form ſeveral air-bubbles, which enlarge its bulk without adding to its weight; this ice, when put into a matraſs, after the manner in which all the preceding experiments with ſalts were made, would elevate the water moſt upon the firſt immerſion: the water would ſink as the ice melted; equal portions of ice would produce equal elevations both before and after ſolution; the air would be ſeparated in a form more or leſs viſible, according to the circumſtances in which the experiment ſhould be tried; and not the ſmalleſt portion of ice could be diſſolved without increaſing the bulk of the whole. Salts do not ſeem to differ much from ice in the manner of their formation, and as ſimilar phænomena attend their ſolution in water, why may we not explain them from the ſame cauſe? But if any one ſhould think differently, notwithſtanding the experiments which have been produced, I profeſs myſelf extremely ready to liſten to any reaſoning founded upon experiment which may tend to prove my opinion to be erroneous; having no partiality for any thing but truth, nor being aſhamed of ignorance or miſtake in any matter, reſpecting the comprehenſion or explication of even the minuteſt operation of nature: *ego quidem hoc ſum contentus, quod licet quo quidque fiat ignorem, quid fiat intelligo.*